

are now ventured as the result of perusing pp. 42-48, which relate to the discontinuity of certain colour-variations.

Without attempting to discuss Mr. Bateson's general propositions, I desire to point out that the facts related in the portion of the work cited, and the "many similar cases" which might be added, do not altogether support the idea of *discontinuous progressive colour-variation*, as distinguished from *atavism*.

Various writers, including myself, have on sundry occasions endeavoured to demonstrate that both in plants and animals a definite succession of colours may be observed. In flowers, for instance, from pink to purple, and from yellow to red; in birds and insects, and many molluscs, the yellow to red succession is commonly observed. In such instances as these, it has been held that one colour represents a lower stage of evolution or a less degree of metabolism than the other; and it has been many times pointed out, that *discontinuous atavistic variation*, e.g. from red to yellow, is commonly to be seen.

Now I take it that Mr. Bateson considers the evidence which he adduces, to illustrate the frequency of *discontinuous progressive variation* in colour, not merely *reversion*. Let us examine this evidence a little more closely.

According to the views held by the writers above mentioned, red is "higher" than yellow, and red varying to yellow is *reversion*. Such *reversion* is well known to be often discontinuous, as in the yellow-fruited yew, the yellow tomato, the yellow-fruited raspberry, the yellow varieties of various red moths, and so forth.

But is the yellow to red variation, which is supposed to be the progressive one, discontinuous? Let Mr. Bateson himself tell us. On p. 45 he cites the variations of the yellow *Gonepteryx rhamni* towards orange. Are these discontinuous? Do we find among the yellow *rhamni* some that are entirely orange? Not so, "there are records of specimens . . . more or less flushed with orange."

Exactly: whereas among the red species of *Callimorpha*, *Arctia*, *Zygæna*, &c., we find varieties not flushed with yellow but entirely yellow in place of red (the dark markings being of course as usual), in the yellow *G. rhamni* we find continuous variation towards orange, none yet having attained actual red.

In birds red species may vary to yellow; green also to yellow, and such variation may be sudden. But yellow to green? or yellow to red? We have got our canary yellow easily enough, but all the art of the breeder cannot get him redder than orange, and the variations thereto are fairly continuous.

We cannot get a blue rose; but the blue *Delphinium*, the blue *Penstemon*, these readily vary to pink. We may have a yellow rose, but it is pretty well agreed that if we ever do see a blue one, it will be by a process of *continuous* variation and selection. Will not Mr. Bateson admit that he would be immensely astonished to see a blue rose arise from seed of a red one, or a scarlet canary from eggs laid by a yellow one? Yet red from blue, or yellow from red, would seem scarcely worth comment in any group of animals or plants, so numerous are the recorded instances of this kind of variation.

On p. 44, Mr. Bateson cites instances of blue in place of red, which should be *progressive variation*. This occurs in *Catocala nupta*, for instance, but *very rarely*, and instances which seem rather intermediate are on record. Another sample cited is the blue-flowered *Anagallis arvensis*. Here the case is different, for the blue and red varieties are entirely distinct, and come true by seed. I have myself lived in districts where the blue and red varieties respectively abounded, and in neither locality did I ever see intermediates. They had all the appearance of true species, which they have often, I think with justice, been considered. The locality for the blue variety was Funchal, Madeira, and there the red pimpernel also occurs. But in England, where the red variety is so common, I never saw the blue one truly wild.

In *Primula* we have yellow species and red species, and, as everyone knows, our common primrose may vary to red. But also, as everyone knows, the variation is continuous. How well I remember as a child looking for those that were tinged with red, always hoping to get one redder than that last found!

The subject admits of much greater amplification than is now possible; and it is by no means denied that many instances may be selected, out of the thousands available which appear to indicate discontinuous progressive colour-variation. But never-

theless, taking the evidence as a whole, I will venture to urge the validity of the following statements:—

(1) Colour-variation occurs in a definite order, the colours forming one or more series.

(2) Variation from those lower to those higher in the scale of evolution, or from those representing less to those representing greater metabolism, is usually continuous.

(3) Reversion from a higher to a lower colour is usually discontinuous.

T. D. A. COCKERELL.
Las Cruces, New Mexico, U.S.A., June 1.

Niagara River since the Ice Age.

MUCH new light on the Quaternary history of the great lakes tributary to the St. Lawrence river has been contributed in three recent papers by Mr. F. B. Taylor, all published within the short time since Mr. G. K. Gilbert's writing on "The Niagara River as a Geologic Chronometer," which appeared in *NATURE* for May 17 (page 53). These papers are in the *Bulletin of the Geological Society of America* (vol. v. pp. 620-626, April 30, 1894), and in the *American Geologist* (vol. xiii. pp. 316-327 and 365-383, May and June, 1894). Supplementing the earlier observations and studies of Whittlesey, Newberry, Gilbert, Spencer, Lawson, Leverett, Wright, Baldwin, and the present writer, among others, these latest explorations and discussions by Mr. Taylor enable us to form a very definite and closely connected historical statement of the relationships of the ice-dammed lakes which preceded the present Laurentian lakes, and of their dependence on the gradual departure of the ice-sheet and on the accompanying northward uplift of that region.

The largest element of uncertainty in the estimate of 7000 years for the Post-glacial period, from the retreat of the ice-sheet to the present time, drawn from the rate of recession of the Falls of Niagara, consists, as Mr. Gilbert has shown, in the probability or possibility that for some considerable time, next following the melting away of the ice upon the area crossed by the Niagara river, the outlet of lakes Superior, Michigan, and Huron may have passed to the St. Lawrence by a more northern course, flowing across the present watershed east of lake Nipissing to the Mattawa and Ottawa rivers. Mr. Taylor's observations now indicate, however, if interpreted on the hypothesis of glacial lakes (which is believed by Mr. Gilbert and by the majority of other geologists of America to be the true view), that the glacial lake Warren, filling the basins of Superior, Michigan, Huron, and Erie, continued with its outlet flowing past Chicago to the Des Plaines, Illinois, and Mississippi rivers, while the country including lake Superior, the northern part of lake Huron, and lake Nipissing, that is, the whole northern side of lake Warren, was uplifted about 350 to 450 feet along its extent of 600 miles from east to west. The existence of lake Warren was terminated by the recession of the ice-sheet from the area between lakes Erie and Ontario, when the Niagara river began to flow and to channel the gorge six miles long below its receding falls, from which the computation for the time since the Ice Age is derived. The Niagara gorge measures the time after the outflow past Chicago ceased, lake Warren being then succeeded in the basins of the upper lakes, above Erie, by the glacial lake Algonquin, while in the Ontario basin the ice-bound lake Iroquois outflowed past Romé, N. Y., by way of the Mohawk and Hudson to the sea.

Seven-eighths of the differential uplifting which carried the watershed east of lake Nipissing above the level of lake Algonquin had taken place before the north-eastward retreat of the ice-sheet uncovered the Niagara area. For some later time the ice-barrier must have remained upon the Mattawa and Ottawa areas, forbidding any outflow there from lake Algonquin; and it seems very probable that within that time the continuation of the uplift had raised the watershed so high that no discharge from the upper lakes ever passed over it. During the ensuing existence of lake Iroquois the Ontario basin was undergoing a rapid northward uplift, which doubtless was shared by the Nipissing area, so that if any outflow occurred there it must have been very brief, being ended when the land east of lake Nipissing rose higher than the present course of outflow by the St. Clair and Detroit rivers to the Erie basin and Niagara river. The duration of the outlet to the Mattawa could probably have been only a few hundred years, at the longest, if it ever existed. With this possible exception, the present volume of the Niagara river has been maintained during all the time of its gorge ero-

sion. Only an insignificant addition to the estimate of 7000 years can therefore be required by the diversion of the waters of the upper lakes.

The view held by Taylor, Spencer, and Lawson, that the high shore lines around the great Laurentian lakes are of marine formation, seems to be inconsistent with the total absence of marine fossiliferous beds overlying the glacial drift in all that region. So far as the sea did extend, after the further recession of the ice-sheet permitted it to come into the St. Lawrence and Ottawa valleys and into the basin of lake Champlain, marine fossils abound; but none are found above the Thousand Islands at the mouth of lake Ontario. We may therefore confidently accept the Niagara gorge as a measure of all the time since the departure of the ice-sheet from the northern United States.

In a recent paper in the *Journal of Geology* (vol. ii. p. 142, February-March, 1894) Mr. Andrew M. Hansen, of Norway, notes the approximate concurrence of about thirty independent measurements and estimates of the duration of the Post-glacial period, which have been made in North America and in Europe, all coming within the limits of 5000 and 12,000 years. He accordingly says: "With full regard to a legitimate calculation of probabilities, it may be predicated that the number of 7000 to 10,000 years is as nearly an exact estimate of the duration of Post-glacial time as can ever be expected."

Minneapolis, Minn., June 9.

WARREN UPHAM.

The Teeth and Civilisation.

NONE of the writers of the interesting letters which have appeared upon this subject seem to have kept before them a distinction which is of the utmost importance in its investigation, and which I should like to state, in order that the attention of those who, like Mr. Wenyon, have opportunities of observation of any segregated community, may be drawn to it.

Dental caries is very prevalent, and its increase seems to be very rapid, so that the last few generations show a marked increase; at least so it is generally believed.

But its victims may be divided into two groups, namely, those whose teeth are apparently perfect in their construction, but nevertheless fall a prey to caries, and those whose teeth show, to the trained eye, clear evidence of structural weakness.

As the latter class present a problem in heredity, and for various reasons are likely to be more interesting to the readers of NATURE than the former, I will dismiss these with a very few words.

There is good reason for supposing that the proximate cause is to be found in vitiation of the oral secretions, as caries often occurs in an extreme degree after diseases of the digestive tract, and examples such as those quoted by Mr. Wenyon are probably to be explained as due to dyspepsia induced by the unhealthy way of feeding.

To the explanation that the enamel may be cracked by alternations of temperature which could be borne in the mouth, I am not inclined to attach importance. In the first place, as a matter of experiment, I have failed to crack enamel by plunging teeth alternately into boiling and ice-cold water, and, as a matter of clinical experience, teeth do not decay along the cracks which from some cause are common in the enamel, but in natural pits of larger size, or on surfaces of contact; the cause is to be sought either in decreased power of resistance, or in the intensification of deleterious influences.

Abnormal conditions of life are known to deleteriously affect the teeth of animals; for example, stall-fed beasts are more liable to diseased conditions in the mouth than those which are fed up in rich pastures; and it has been pointed out by Mr. Bland Sutton, that some animals in the Zoological Gardens suffer in this way, notably the Lemurs, whose teeth frequently loosen and fall out.

It is rare to meet with good teeth in children whose parents have had bad teeth, and peculiarities of form in the teeth and jaws are often inherited with curious exactitude. But it is quite common to meet with instances of healthy parents with good teeth bearing a family of children, also apparently healthy and well-grown, whose teeth, although to the casual observer normal in shape, size, and general aspect, are to the eye of the dentist doomed to early destruction, and speedily undergo it.

These teeth have an appearance somewhat difficult to describe; they have a glassy look, are more translucent than they should be, are softer, and are believed, though the proof is not com-

plete, to be somewhat deficient in their proper proportion of lime salts. This kind of tooth is very apt to run through a whole family, and its causes must be sought either in some condition of inheritance, or if it be due to anything acting upon the individual, it must be something which commences to act immediately after birth. Comparative anatomy teaches us that the teeth are less variable than the jaws. In long-muzzled dogs the teeth are spaced; in short-muzzled dogs they are crowded, reduction in size having gone on faster in the bones than in the teeth; and the same thing is observed in the human subject.

Moreover in animals, whilst some variability is often observable in the teeth, that variability does not take the form of structural difference, but only of differences of size and shape.

Again, in rickets, where the bones are starved of lime salts, the teeth contrive to get more than their share; it is therefore not a little remarkable that we should find the teeth, and apparently the teeth alone, to have deteriorated in one generation.

On the other hand, it is equally difficult to find any cause which shall have operated alike upon all the children of a family if we reject inheritance as being at the bottom of it; it does not appear to be determined by the greater frequency of hand-feeding, as I know of instances in the same family where some children were nursed and others were not, and yet their teeth were alike of the poor structure to which I have alluded.

Upon the whole, I am rather inclined to attribute it to some causes operating shortly after birth, rather than before it, for the milk teeth, which are well started in utero, are far less liable to structural variation than are the permanent teeth, whose calcification is mainly effected after birth; but I need hardly say that the period of occurrence does not by any means exclude inheritance.

However the question, interesting and important as it is, is not so simple as some of your correspondents imagine, and there is a considerable amount of literature upon the subject which seems to have escaped them, some of it accurate and valuable, some of it quite the reverse.

CHARLES S. TOMES.

Electrical Theory of Vision.

IN my letter to Prof. Lodge, published in your last issue, I notice a printer's error, which I think should be corrected, as it gives an entirely wrong meaning to the sentence in which it occurs. As it stands it reads as follows: "The energy thus lost by the tissues was then *suppressed* from without by the vibrating fingers," whereas I said the energy was *supplied* from without by the fingers, the idea being that the shaking back of the eyes to their normal state of rest, evinced by the sensation of darkness, is perfectly analogous to the tapping back of Prof. Lodge's "Coherer" to its normal position, evinced by the return of the galvanometer needle to zero.

E. OBACH.

Old Charlton, Kent, June 23.

CLIMBING AND EXPLORATION IN THE KARAKORAM-HIMALAYAS.¹

THE mountain district explored by Mr. Conway lies on the southern side of the watershed of the Karakoram chain, and is drained by tributaries of the Upper Indus. For most of the time he was in Baltistan, but ended his journey by a visit to Leh. Here, at about 11,500 feet above sea-level, is a small meteorological observatory, which enabled Mr. Conway to check his observations by a comparison of barometers. Besides himself, the party consisted of the Hon. C. G. Bruce, Mr. McCormick, the artist, Messrs. Eckenstein and Roudebush, Matthias Zurbriggen, the well-known guide from Macagna, four Gurkas, who took readily to ice work, and one or two other native attendants, with, of course, a considerable but variable party of coolies.

As the author states, the party spent in all eighty-four days on snow or glacier. They were often encamped at

¹ "Climbing and Exploration in the Karakoram-Himalayas." By William Martin Conway, M.A., F.S.A., &c. With 300 Illustrations by A. P. McCormick, and a Map. (London: T. Fisher Unwin, 1894.)